

Hot spots policing and crime prevention: A systematic review of randomized controlled trials

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Abstract. Crime scholars and practitioners have argued that police actions should be focused on high-risk crime places rather than spread thinly across the urban landscape. This review examines five randomized controlled trials of the effects of concentrating police enforcement efforts on crime hot spots. The findings of these evaluations suggest that focused police actions can prevent crime and disorder in crime hot spots. A meta-analysis of the effect sizes from the five experiments reveals a statistically significant mean effect size for hot spots policing interventions; this suggests overall reductions in citizen calls for service in the treatment hot spots relative to the control hot spots. These studies also suggest that focused police actions at specific locations do not necessarily result in crime displacement. Although these evaluations reveal that these programs work in preventing crime, additional research is needed to unravel other important policy-relevant issues such as community reaction to focused police enforcement efforts.

Key words: crime prevention, experiment, hot spots, meta-analysis, police, systematic review

Introduction

In recent years, crime scholars and practitioners have pointed to the potential benefits of focusing crime prevention efforts on crime places. A number of studies suggest that crime is not spread evenly across city landscapes. Rather, there is significant clustering of crime in small places, or “hot spots,” that generate half of all criminal events (Pierce et al. 1988; Sherman et al. 1989a, b; Weisburd et al. 1992). Even within the most crime-ridden neighborhoods, crime clusters at a few discrete locations and other areas are relatively crime-free (Sherman et al. 1989b). A number of researchers have argued that many crime problems can be reduced more efficiently if police officers focused their attention on these deviant places (Sherman and Weisburd 1995; Weisburd and Green 1995a). The appeal of focusing limited resources on a small number of high-activity crime places is straightforward. If we can prevent crime at these hot spots, then we might be able to reduce total crime.

Hot spots policing has become a very popular way for police departments to prevent crime. A recent Police Foundation report found that 7 in 10 departments with more than 100 sworn officers reported using crime mapping to identify crime hot spots (Weisburd et al. 2003). A growing body of research evidence suggests that focused police interventions, such as directed patrols, proactive arrests, and

problem-oriented policing, can produce significant crime prevention gains at high-crime “hot spots” (see, e.g., Braga 2002; Eck 1997, 2002; Skogan and Frydl 2004; Weisburd and Eck 2004). Given the growing popularity of hot spots policing, regular systematic reviews of the empirical evidence on the effects of focused police interventions on crime hot spots are necessary to assess the value of this approach to crime prevention.

This research study presents an updated systematic review of the effects of hot spots policing on crime. An earlier review of randomized controlled experiments and quasi-experiments concluded that hot spots policing was effective in preventing crime based on a simple vote-counting procedure (Braga 2001). Seven of nine identified studies reported noteworthy crime prevention gains associated with hot spots policing interventions. Due to inconsistent reporting of program effects in the quasi-experimental studies, the earlier review did not include a meta-analysis.¹ This updated review was conducted as part of a Smith Richardson Foundation sponsored effort by the Campbell Collaboration’s Crime and Justice Group to review experimental evidence on criminal justice policy interventions (<http://www.aic.gov.au/campbellcj/>). Therefore, the current review was limited to randomized experiments and this constraint permitted a meta-analysis of program effects based on the identified studies.

Background of the intervention

Unlike most innovations in policing, which are normally based on increasing operational and management efficiency, the emergence of hot spots policing can be traced directly to emerging theoretical perspectives in criminology that suggest the importance of places in understanding crime (Weisburd and Braga 2003). The consideration of such place-oriented strategies in crime control policy arose from research suggesting that micro-level variation in crime existed within communities. The observation that the distribution of crime varied within neighborhoods has existed for some time (see Hawley 1944, 1950; Shaw and McKay 1942; Werthman and Piliavin 1967); however, until recently, little research examined this variance beyond the community level of analysis. With the advent of powerful computer systems and software packages, several studies revealed that over half of all crimes in a city are committed at a few criminogenic places within communities (Pierce et al. 1988; Sherman et al. 1989a, b). Further, research by Taylor and Gottfredson (1986) suggests that conclusive evidence links this variation to physical and social characteristics of particular blocks and multiple dwellings within a neighborhood. This uneven distribution of crime within specific neighborhoods has been reported in studies of a variety of crime types including drug selling (Weisburd and Green 1994), burglary (Pease 1991), robbery (Hunter and Jeffrey 1992), and auto theft (Clarke and Harris 1992).

Beyond studies observing the *clustering* of criminal events, in their review of the research literature, Eck and Weisburd (1995) identified four other theoreti-

cal concepts that illuminate the role of place in crime. *Facilities*, such as bars, churches, and apartment buildings have been found to affect crime rates in their immediate environment depending on the type of people attracted, the way the space is managed, or the possible crime controllers present such as owners, security, or police. *Site features* such as easy access, a lack of guardians, inept or improper management, and the presence of valuable items have been suggested to influence the decisions offenders make about the place they choose to commit their crimes. Studies of *offender mobility* suggest that offenders' target searching behavior is influenced by personal characteristics (such as gender, age, race, experience, and crime types) and the distribution of crime targets. A direct outgrowth of offender mobility patterns, research on *target selection* posits that offenders seek places with cues that indicate acceptable risks and gains, such as homes on the outskirts of affluent neighborhoods; these places are found during intentional target searches and during their daily legitimate routines.

The study of crime events at places is influenced and supported by three complementary theoretical perspectives: rational choice, routine activities, and environmental criminology. The rational choice perspective assumes that "offenders seek to benefit themselves by their criminal behavior; that this involves the making of decisions and choices, however rudimentary on occasion these choices may be; and that these processes, constrained as they are by time, the offender's cognitive abilities, and by the availability of relevant information, exhibited limited rather than normative rationality" (Cornish and Clarke 1987: 933). This perspective is often combined with routine activity theory to explain criminal behavior during the crime event (Clarke and Felson 1993). Routine activities theory posits that a criminal act occurs when a likely offender converges in space and time with a suitable target (e.g., victim or property) in the absence of a capable guardian (Cohen and Felson 1979). Rational offenders come across criminal opportunities as they go about their daily routines and make decisions whether to take action. The assumption is that, if victims and offenders are prevented from converging in space and time through the effective manipulation of the situations and settings that give rise to criminal opportunities, police can reduce crime.

Environmental criminology explores the distribution and interaction of targets, offenders, and opportunities across time and space; understanding the characteristics of places, such as facilities, is important as these attributes give rise to the opportunities that rational offenders will encounter during their routine activities (Brantingham and Brantingham 1991). Although this perspective is primarily concerned with applied crime prevention, Weisburd et al. (1992) suggest "environmental criminology's basic contribution lay in its call for a change in the unit of analysis from persons to places" (48). The attributes of a place are viewed as key in explaining clusters of criminal events. For example, a poorly lit street corner with an abandoned building, located near a major thoroughfare, provides an ideal location for a drug market. The lack of proper lighting, an abundance of "stash" locations around the derelict property, a steady flow of potential customers on the thoroughfare, and a lack of informal social control (termed

defensive ownership) at the place generates an attractive opportunity for drug sellers. In many such cases, the police spend considerable time and effort arresting sellers without noticeably impacting the drug trade. The compelling criminal opportunities at the place attract sellers and buyers, and thus sustain the market. If the police want to be more efficient at disrupting the market, this suggests they should focus on the features of the place which cause the drug dealing to cluster at that particular location (see, e.g., Green 1996). This perspective is considered a radical departure from traditional criminological theories that focused prevention efforts on the individual and ignored the importance of place (Weisburd 1997; Sherman et al. 1989b).

Indeed, police officers have long recognized the importance of place in crime problems. Police officers know the locations within their beats that tend to be trouble spots and also are often very sensitive to signs of potential crimes across the places that comprise their beats. As Bittner (1970: 90) suggests in his classic study of police work, some officers know “the shops, stores, warehouses, restaurants, hotels, schools, playgrounds, and other public places in such a way that they can recognize at a glance whether what is going on within them is within the range of normalcy.” The traditional response to such trouble spots typically included heightened levels of patrol and increased opportunistic arrests and investigations. Until recently, police crime prevention strategies did not focus systematically on crime hot spots and did not seek to address the underlying conditions that give rise to high-activity crime places. Many police departments now report having the capability to manage and analyze crime data in sophisticated ways and, through management innovations such as Compstat, hold officers accountable for implementing problem-solving strategies to control hot spot locations (Weisburd et al. 2003). The widespread use of hot spots policing to prevent crime warrants a careful review of the available empirical evidence on the crime control benefits of the approach.

Methods used in systematic review

This review synthesizes existing published and non-published empirical evidence on the effects of focused police crime prevention interventions at crime hot spots and provides a systematic assessment of the preventive value of these programs. In keeping with the conventions established by the systematic reviews methods literature, the stages of this review and the criteria used to select eligible studies are described below.

Types of studies

This review was limited to studies that used randomized controlled trial designs. In many hot spots policing experiments (e.g., Braga et al. 1999; Weisburd and Green 1995a), the control group experiences routine police interventions (e.g., regular levels of random patrol, *ad hoc* investigations, etc.). In eligible studies, crime

places that received the hot spots policing intervention were compared to places that experienced routine levels of traditional police service.

Types of areas

The units of analysis were crime hot spots or high-activity crime “places.” As Eck (1997: 7–1) suggests, “a place is a very small area reserved for a narrow range of functions, often controlled by a single owner, and separated from the surrounding area . . . examples of places include stores, homes, apartment buildings, street corners, subway stations, and airports.” All studies using units of analysis smaller than a neighborhood or community were considered. This constraint was placed on the review process to ensure that identified studies were evaluating police strategies focused on the small number of locations that generate a disproportionate amount of crime in urban areas. As described earlier, hot spots policing was a natural outgrowth of theoretical perspectives that suggested specific places where crime concentrates were an important focus for strategic crime prevention efforts. Police interventions implemented at the community or neighborhood level would not be specifically focused on small places, often encompassing only one or a few city blocks, that would be considered hot spots of crime.

The methodological approaches used to identify hot spots in the eligible studies were also reviewed. Diverse types of hot spots may respond to treatment in different ways. As such, the review needed to be sensitive to varying hot spot identification methods that could influence whether or not the treatment generated crime prevention gains.

Types of interventions

To be eligible for this review, interventions used to control crime hot spots were limited to police enforcement efforts. Suitable police enforcement efforts included traditional tactics such as directed patrol and heightened levels of traffic enforcement as well as alternative strategies such as aggressive disorder enforcement and problem-oriented policing with limited situational responses and limited engagement of the public. Eligible problem-oriented policing initiatives must engage primarily traditional policing tactics such as law enforcement actions, informal counseling and cautioning, and referrals to other agencies. Problem-oriented policing programs that involved multiple interventions implemented by other stakeholders such as community members, business owners, or resident managers, were not considered.

Studies of police crackdown programs were also considered (see, e.g., Sherman 1990). However, to be included in the review, crackdown programs had to be focused on very specific places and not limited to a one-time only swamping of

police resources. Some ongoing attention to crime hot spots must be a characteristic of the program whether it was a series of subsequent crackdowns or simple maintenance of the targeted area through other means (e.g., additional follow-up directed patrol). This inclusion criterion ensured that only crackdown programs that were similar to more formal hot spots policing programs were considered.

Types of outcome measures

Eligible studies had to measure the effects of police intervention on officially recorded levels of crime at places. Appropriate measures of crime included crime incident reports, citizen emergency calls for service, or arrest data. Other outcomes measures such as survey, interview, social observations, physical observations, and victimization measures used by eligible studies to measure program effectiveness were also coded and analyzed.

Particular attention was paid to studies that measure crime displacement effects and diffusion of crime control benefit effects. Policing strategies focused on specific locations have been criticized as resulting in displacement (see Repetto 1976). More recently, academics have observed that crime prevention programs may result in the complete opposite of displacement – that crime control benefits were greater than expected and “spill over” into places beyond the target areas (Clarke and Weisburd 1994). The quality of the methodologies used to measure displacement and diffusion effects, as well as the types of displacement (spatial, temporal, target, modus operandi) examined, was assessed.

Search strategies for identification of studies

To identify studies meeting the criteria of this review, the following four search strategies were used:

1. Searches of on-line databases (see below)
2. Searches of narrative and empirical reviews of literature that examine the effectiveness of police interventions on crime hot spots (e.g., Braga 2001; Eck 1997, 2002; Sherman 1990, 1997)
3. Searches of bibliographies of police crime prevention efforts and place-oriented crime prevention programs (e.g., Braga 2002; Sherman 2002)
4. Contacts with leading researchers

These search strategies complemented each other in the identification of eligible hot spots policing studies. For example, if an eligible study existed that did not appear in one of the on-line databases, contacts with leading researchers and searches of existing bibliographies were likely to discover the study in question. All published and unpublished studies were considered for this review. Each on-line database was searched as far back as possible. However, since hot spots

policing is a very recent development in crime prevention, the search strategies described above should be sufficient to identify all relevant studies.

The following 11 databases were searched:

1. Criminal Justice Periodical Index
2. Sociological Abstracts
3. Social Science Abstracts (SocialSciAbs)
4. Social Science Citation Index
5. Arts and Humanities Search (AHSearch)
6. Criminal Justice Abstracts
7. National Criminal Justice Reference Service (NCJRS) Abstracts
8. Educational Resources Information Clearinghouse (ERIC)
9. Legal Resource Index
10. Dissertation Abstracts
11. Government Publications Office, Monthly Catalog (GPO Monthly)

The following terms were used to search the ten databases listed above:

1. Hot spot
2. Crime place
3. Crime clusters
4. Crime displacement
5. Place-oriented interventions
6. High crime areas
7. High crime locations
8. Targeted policing
9. Directed patrol
10. Crackdowns
11. Enforcement swamping

In addition, two existing registers of randomized controlled trials were consulted. These included (1) the “Registry of Experiments in Criminal Sanctions, 1950–1983” (Weisburd et al. 1990) and (2) the Campbell Collaboration Social, Psychological, Educational, and Criminological Trials Register” or C2-SPECTR now maintained by the University of Pennsylvania (Turner et al. 2003).

Findings of the review

Selection of studies

Search strategies in the systematic review process generate a large number of citations and abstracts for potentially relevant studies that must be closely screened to determine whether the studies meet the eligibility criteria (Farrington and Petrosino 2001). The screening process yields a much smaller pool of eligible studies for inclusion in the review. In May 2003, the four

Table 1. Results of the on-line database search.

<i>Term</i>	<i>Criminal justice periodical index (1975–1998)</i>	<i>Sociological abstracts [Sociofile] (1974–2003)</i>	<i>Social science abstracts [SocialSciIndex]</i>	<i>Social science citation index (1956–present)</i>
Hot spot	19	3	38	29
Crime place	0	8	219	2
Crime clusters	1	1	7	0
Crime displacement	5	7	12	9
Place-oriented interventions	0	0	0	0
High crime areas	5	22	62	20
High crime locations	0	0	2	0
Targeted policing	0	0	7	4
Directed patrol	5	3	5	3
Crackdowns	6	11	16	13
Enforcement swamping	0	0	0	1
	<i>Arts & humanities citation index (1975–present)</i>	<i>Criminal justice abstracts^a</i>	<i>NCJRS abstracts (500 max)</i>	<i>ERIC (1966–May 2003)</i>
Hot spot	7	70	143	3
Crime place	0	20	500	1
Crime clusters	0	2	243	0
Crime displacement	0	25	250	0
Place-oriented interventions	0	2	33	0
High crime areas	0	102	500	10
High crime locations	0	4	331	0
Targeted policing	0	6	500	0
Directed patrol	0	23	232	0
Crackdowns	0	62	127	2
Enforcement swamping	0	1	0	0
	<i>Legal Resource Index [Legaltrac]</i>	<i>Dissertation Abstracts</i>	<i>Government Publications Office (enhanced GPO catalog)</i>	
Hot spot	9	228	76	
Crime place	4	0	3	
Crime clusters	0	1	0	
Crime displacement	3	1	0	
Place-oriented interventions	0	0	0	
High crime areas	8	4	2	
High crime locations	0	0	0	
Targeted policing	0	0	1	
Directed patrol	0	3	2	
Crackdowns	4	5	0	
Enforcement swamping	0	0	0	

^aThe numbers in the Criminal Justice Abstracts search reflect the numbers of abstracts associated with the exact phrase (i.e., “crime place” rather than “crime” & “place”). For the Criminal Justice Abstracts search, the term “prevention” was added for a closer search of the database (i.e., “crime place” & “prevention”). This resulted in an additional 205 abstracts (includes the 20 reported in the table). If “crime” & “place” were run separately, the total number of abstracts would have been 907.

search strategies produced 697 distinct abstracts (see Table 1 for results of on-line database search).² 57 distinct abstracts were selected for closer review and the full-text reports, journal articles, and books for these abstracts were acquired and carefully assessed to determine whether the interventions involved focused police enforcement efforts at crime hot spots and whether the studies used randomized controlled trial designs. Five eligible studies were identified and included in this review: the Minneapolis Repeat Call Address Policing (RECAP) Program (Sherman et al. 1989a), the Minneapolis Hot Spots Patrol Program (Sherman and Weisburd 1995), the Jersey City Drug Markets Analysis Program (DMAP) (Weisburd and Green 1995a), the Jersey City Problem-Oriented Policing at Violent Places Project (Braga et al. 1999), and the Kansas City Crack House Police Raids Program (Sherman and Rogan 1995a). The updated review did not identify any new experimental (or quasi-experimental) evaluations of hot spots policing conducted since the original 2001 review (Braga 2001).

Characteristics of selected studies

The five randomized controlled trials were conducted in three large cities in the United States and completed between 1989 and 1999. Research teams involving either Lawrence W. Sherman or David L. Weisburd conducted all the evaluations. The treatments used to prevent crime at hot spots fell into three broad categories: enforcement problem-oriented policing interventions, directed and aggressive patrol programs, and police crackdowns and raids (see Table 2). The effects of problem-oriented policing initiatives comprised of mostly traditional tactics with limited situational responses were evaluated in the Minneapolis RECAP Program and Jersey City POP at Violent Places Studies. The Minneapolis Hot Spots Patrol program evaluated the effects of increased levels of preventive patrol on crime. The Jersey City DMAP and Kansas City Crack House Raids Programs evaluated the effects of well-planned crackdowns on street-level drug markets and court authorized raids on crack houses, respectively.

All randomized experiments used crime hot spots as the units of analysis. The Minneapolis Hot Spots Patrol, Jersey City DMAP, and Jersey City POP at Violent Places experiments used very detailed methods to identify hot spots. In general, the research teams defined hot spot areas by mapping official police call data to identify high volume street address clusters and intersection areas, ensured that these locations had stable numbers of calls over time, and considered qualitative indicators such as police and researcher observations to define hot spot boundaries. The Kansas City Crack House Raid experiment focused on blocks that had at least five calls for service in the month preceding an undercover drug buy made on the inside of a residence located on the block. Simple ranking procedures to identify high-volume addresses based on numbers of citizen calls for service were used to define specific locations for focused police interventions in the Minneapolis RECAP experiment.

Table 2. Hot spots policing experiments.

<i>Study</i>	<i>Treatment</i>	<i>Hot spot definition</i>	<i>Research design^a</i>
Minneapolis (MN) RECAP Sherman et al. (1989a)	Problem-oriented policing interventions comprised of mostly traditional enforcement tactics with some situational responses; 1 year intervention period; Integrity of treatment threatened by large caseloads that outstripped the resources the RECAP unit could bring to bear	Addresses ranked by frequency of citizen calls for service divided into commercial and residential lists; the top 250 commercial and top 250 residential addresses were included in experiment	Randomized experiment; control and treatment groups were each randomly allocated 125 commercial and 125 residential addresses; Differences in the number of calls to each address from a baseline year to the experimental year were compared between RECAP and control groups
Minneapolis (MN) Hot Spots Sherman and Weisburd (1995)	Uniformed police patrol; experimental group, on average, experienced twice as much patrol presence; 1 year intervention period; Breakdown in the treatment noted during the summer months	110 hot spots comprised of address clusters that experienced high volumes of citizen calls for service, had stable numbers of calls for over 2 years, and were visually proximate	Randomized experiment; control and treatment groups were each randomly allocated 55 hot spots within statistical blocks; Differences of differences between citizen calls in baseline and experimental years, comparing control and experimental groups
Jersey City (NJ) DMAP Weisburd and Green (1995a)	Well-planned crackdowns followed by preventive patrol to maintain crime control gains; 15 month intervention period; Slow progress at treatment places caused intervention time period to be extended by 3 months	56 drug hot spot areas identified based on ranking intersection areas with high levels of drug-related calls and narcotics arrests, types of drugs sold, police perceptions of drug areas, and offender movement patterns	Randomized experiment; control and treatment groups were each randomly located 28 drug hot spots within statistical blocks; Differences of differences between citizen calls during 7 month pre-test and post-test periods, comparing control and experimental groups

Jersey City (NJ) POP at Violent Places Braga et al. (1999)	Problem-oriented policing interventions comprised of mostly aggressive disorder enforcement tactics w/ some situational responses; 16 month intervention period; Initial slow progress at places caused by resistance of officers to implement intervention	24 violent crime places identified based on ranking intersection areas with high levels of assault and robbery calls and incidents, and police and researcher perceptions of violent areas	Randomized experiment; 24 places were matched into like pairs based on simple quantitative and qualitative analyses; control and treatment groups were each randomly allocated 12 places within matched pairs; Differences of differences between a number of indicators during 6 month pre-test and post-test periods, comparing control and experimental xgroups
Kansas City (MO) Crack House Raids Sherman and Rogan (1995a)	Court authorized raids on crack houses conducted by uniformed police officers; Intervention period was the day of the raid; All but seven cases received randomly assigned treatment as assigned; No threats to the integrity of the treatment reported	207 blocks with at least five calls for service in the 30 days preceding an undercover drug buy; sample was restricted to raids on the inside of residences where a drug buy was made that was eligible for a search warrant	Randomized experiment; Raids were randomly allocated to 104 blocks and were conducted at 98 of those sites; the other 109 blocks did not receive raids; Differences of differences analytic design; pre-post time periods were 30 days before and after raid for experimental blocks, and 30 days before and after controlled buy at treatment block for control blocks

^a The control group in each study received routine levels of traditional police enforcement tactics.

Effects of hot spots policing programs on crime and disorder

Noteworthy crime reductions were reported in four of the five selected studies (see Table 3). In the Jersey City POP experiment, the enforcement problem-oriented policing strategy resulted in statistically significant reductions in total calls for service and total crime incidents, as well as varying reductions in all subcategories of crime types, in the treatment violent crime hot spots relative to controls (Braga et al. 1999). Analyses of systematic observation data collected during the pre-test and post-test periods revealed that social disorder was alleviated at 10 of 11 treatment places relative to controls.³ Non-experimental systematic observation data (outcome data collected in the treatment places only) suggested that physical disorder was alleviated at 10 of 11 treatment places.⁴ Pre-test and post-test interviews with key community members suggested that community perceptions of places improved at 7 of 12 treatment places; however, a binomial test of these data suggested that the observed improvements were not significantly different from what would be expected if no treatment was applied to the places (Braga 1997: 235–236).

The Minneapolis Hot Spots Patrol experiment revealed that roughly doubling the level of patrol in crime hot spots resulted in modest, but statistically significant, reductions in total calls for service, ranging from 6% to 13%, in treatment places relative to control places (Sherman and Weisburd 1995). Moreover, systematic observations of the hot spots suggested that disorder was only half as prevalent in treatment hot spots as compared to control hot spots. The Jersey City DMAP experiment suggested that well-planned crackdowns followed by patrol maintenance resulted in statistically significant reductions in disorder calls for service at the treatment drug hot spots relative to controls (Weisburd and Green 1995a). The Kansas City Crack House Raid experiment reported modest decreases in citizen calls for service and crime offenses at treatment blocks relative to controls that decayed within 2 weeks of the raids (Sherman and Rogan 1995a).

The Minneapolis RECAP experiment showed no statistically significant differences in the prevalence of citizen calls for service at commercial and residential addresses that received the problem-oriented policing treatment as compared to control addresses (Sherman et al. 1989a). These results were probably due to the assignment of too many cases to the RECAP unit, thus outstripping the amount of resources and attention the police officers provided to each address (Buerger 1993). Moreover, the simple randomization procedure led to the placing of some of the highest event addresses into the treatment group; this led to high variability between the treatment and control groups and low statistical power. Although the overall findings suggest that the RECAP program was not effective in preventing crime, a case study analysis revealed that several addresses experienced dramatic reductions in total calls for service (Buerger 1992).

In addition to the RECAP Experiment, three other studies reported potential threats to the internal validity of the research designs. The Jersey City DMAP experiment and Jersey City POP at Violent Places experiment reported instances where the treatments were threatened by subversion by the participants. The

Table 3. Results of hot spots policing experiments.

<i>Study</i>	<i>Crime outcomes</i>	<i>Other outcomes</i>	<i>Displacement/diffusion</i>
Minneapolis (MN) RECAP Sherman, Buerger, Gartin (1989)	No statistically significant differences in the prevalence of citizen calls for service	None	Not measured
Minneapolis (MN) Hot Spots Sherman and Weisburd (1995)	Modest, but statistically significant reductions in total crime calls for service ranging from 6% to 13%	Systematic observations of crime and disorder were half as prevalent in experimental as in control hot spots	Not measured
Jersey City (NJ) DMAP Weisburd and Green (1995a)	Statistically significant reductions in disorder calls for service in treatment drug markets relative to control drug markets	None	Examined displacement and diffusion effects in two-block catchment areas surrounding the treatment and control drug places and replicated the drug market identification process; Little evidence of displacement; analyses suggest modest diffusion of benefits
Jersey City (NJ) POP at Violent Places Braga et al. (1999)	Statistically significant reductions in total calls for service and total crime incidents All crime categories experienced varying reductions; statistically significant reductions in street fight calls, property calls, narcotics calls, robbery incidents, and property crime incidents	Observation data revealed that social disorder was alleviated at 10 of 11 treatment places relative to control places; Non-experimental observation data revealed that physical disorder was alleviated at 10 of 11 treatment places; Non-experimental interviews with key community members in target locations suggest no noteworthy improvements in citizen perceptions of places	Examined displacement and diffusion effects in two-block catchment areas surrounding the treatment and control drug places; Little evidence of immediate spatial displacement or diffusion
Kansas City (MO) Crack House Raids Sherman and Rogan (1995a)	Modest decreases in citizen calls and offense reports that decayed in 2 weeks	None	Not measured

officers charged with preventing crime at the treatment hot spots were resistant to participating in the programs and this resulted in low levels of treatment during the early months of both experiments. In the Jersey City DMAP experiment, this situation was remedied by providing a detailed crackdown schedule to the Narcotics Squad commander and extending the experiment from 12 months to 15 months. This problem was remedied in the Jersey City POP experiment by changing the leadership of the POP unit, developing an implementation accountability system, providing additional training in the problem-oriented policing approach, and through other smaller adjustments.

The patrol treatment in the Minneapolis Hot Spots experiment was disrupted during summer months due to a peak in the overall calls for service received by the Minneapolis Police Department and a shortage of officers due to vacations; this situation was further complicated by changes in the computerized calls for service system implemented in the fall. The changes in the calls for service system and the disappearance of differences in patrol dosage between treatment and control hot spots during summer months were addressed by conducting separate outcome analyses using different intervention time periods; there were no substantive differences in the outcomes of the experiment across the different time periods. Of course, these implementation problems are not unique to these experiments; many well-known criminal justice field experiments have experienced and successfully dealt with methodological difficulties.⁵

Displacement and diffusion effects

Only two studies, the Jersey City experiments, examined whether focused police efforts were associated with crime displacement or diffusion of crime control benefits (see Table 3). Prior to a discussion of the research findings, it must be noted that it is very difficult to detect displacement effects, because the potential manifestations of displacement are quite diverse (Weisburd and Green 1995b). As Barr and Pease (1990: 293) suggest, "if, in truth, displacement is complete, some displaced crime will fall outside the areas and types of crime being studied or be so dispersed as to be masked by background variation...no research study, however massive, is likely to resolve the issue." Diffusion effects are likely to be as difficult to assess. The Jersey City experiments were limited to examining immediate spatial displacement and diffusion effects; that is, whether focused police efforts in targeted areas resulted in crime "moving around the corner" or whether these proximate areas experienced unintended crime control benefits.

Neither study reported substantial immediate spatial displacement of crime into areas surrounding the targeted locations and both suggested possible diffusion effects associated with the focused police interventions. In the two Jersey City experiments, the research teams examined the differences of differences in citizen calls for service in two block catchment areas surrounding treatment and control hot spot areas. The Jersey City POP at Violent Places experiment found little evidence of displacement in the catchment areas and reported statistically

significant decreases in total calls for service and disorder calls for service in the catchment areas.⁶ The Jersey City DMAP experiment found statistically significant decreases in public morals calls for service and narcotics calls for service in treatment catchment areas relative to controls. The Jersey City DMAP experiment also replicated the drug market identification process and found six new drug hot spots within two blocks of the treatment locations. However, the analysis also revealed that the development of new drug hot spots was twice as likely in areas surrounding the control locations relative to areas surrounding the treatment locations. These results suggest that some modest displacement may have occurred, but it could not be determined whether these new drug hot spots were the result of experimental squad actions, control squad actions, or would have developed naturally without any enforcement efforts. The findings of the Jersey City experiments support the growing body of research evidence that suggests that focused crime prevention efforts do not inevitably lead to the displacement of crime problems (Clarke and Weisburd 1994; Hesselning 1994; Eck 1993).

Meta-analysis of the effects of hot spots policing on crime and disorder

Vote counting methods of synthesizing results across studies, such as the procedure used above and in the earlier review (Braga 2001), suffer from a number of limits. These weaknesses include: failing to account for the differential precision of the studies being reviewed (e.g., larger studies, all else being equal, provide more precise estimates), failing to recognize the fundamental asymmetry of statistical significance tests (e.g., a large number of nonsignificant findings in the same direction provide evidence that the null hypothesis is false), ignoring the size and direction of observed program effects, and, if the statistical power of the studies in that area of concern is low, the likelihood of arriving at an incorrect conclusion increases as the number of studies on a topic increases (Wilson 2001: 73–74). Meta-analyses of program effects avoid these pitfalls by focusing on the size and direction of the effects, not whether the individual effects were statistically significant, and by weighting effect sizes based on the variance of the effect size and the study sample size (Lipsey and Wilson 2001).

The standardized mean difference effect size (also known as Cohen's *d*; see Rosenthal 1994; Shadish et al. 2003) is used in meta-analysis to synthesize results from studies that contrast two groups that have a continuous underlying distribution. Unfortunately, the five randomized controlled trials did not consistently report the necessary data to conduct a meta-analysis of program effects based on calculations of standard mean difference effect size statistics directly from mean differences, standard deviations, and the like (see Lipsey and Wilson 2001). In these situations, standardized mean difference effect size measures can be imputed from *P* levels of statistical significance or from *t*-test statistics reported in the evaluations (Rosenthal 1994). The *r* effect size correlation coefficients (i.e., Pearson's product moment correlation) can be calculated based on reported *P* levels and *t*-statistics and subsequently converted into standardized mean

difference effect sizes (Lipsey and Wilson 2001; Rosenthal 1994). The methodology used in this review to calculate effect sizes was as follows: r was calculated from the reported P level or t -test statistic (Rosenthal 1994: 239); r was then converted to the standardized mean difference (Rosenthal 1994: 239); the standardized mean difference was adjusted by Hedges' correction to account for potential positive bias inherent in small samples (Lipsey and Wilson 2001: 48–49); and the associated standard error and inverse variance weight were calculated (Lipsey and Wilson 2001: 49).

Since all hot spots policing experiments used citizen calls for service as an outcome measure, the main effect size for each study was calculated based on the statistics reported for key calls for service findings. In the Jersey City POP at Violent Places, Kansas City Crack House Raids, and Minneapolis Hot Spots Patrol experiments, the effects of the treatment on total calls for service were used to calculate the effect size measures. In the Jersey City DMAP experiment, the authors examined the effects of the treatment on varying call subcategories and reported the effects of the treatment on disorder calls for service as the key finding of the study. As such, the effect size for disorder calls was included as the main outcome measure. The Minneapolis RECAP experiment reported the effect of the treatment on total calls for service separately for the 250 residential addresses and the 250 commercial addresses. Since the residential and commercial addresses represented distinct locations, two independent effect sizes for total calls for service were calculated for the RECAP experiment. These data were coded, entered, and analyzed using SPSS statistical software.⁷ Following social science convention, the two-tailed 0.05 level of significance was selected as the benchmark to reject the null hypothesis of “no difference.”

Table 4 presents the standardized mean difference effect sizes, standard errors, inverse variance weights, and 95% confidence intervals for the hot spots experiments. The effect size of the hot spots policing intervention on the treatment places relative to control places was very large (2.05) and statistically significant in the Jersey City POP at Violent Places experiment. While the Jersey City POP experiment reported a very large effect size, the influence of the study on the overall meta-analysis was moderated by its small sample size and correspondingly small inverse variance weight (only 1.8% of the total weight). The Jersey City DMAP experiment intervention also had a large statistically significant effect size (0.689) and the Minneapolis Hot Spots Patrol experiment intervention had a moderate statistically significant effect size (0.322). The Kansas City Crack House Raid experiment and the Minneapolis RECAP experiment commercial addresses interventions had smaller non-statistically significant effect sizes that favored the treatment places relative to the controls (0.219 and 0.089, respectively). The Minneapolis RECAP experiment residential addresses intervention had a very small, non-statistically significant effect size that slightly favored the control places relative to the treatment places. The distribution of effect sizes was found to be heterogeneous when the Q statistic was considered ($Q = 20.53$, $df = 5$, $P = 0.001$, Random effects variance component (ν) = 0.09184).⁸ Therefore, a random effects meta-analytic model was used to calculate the mean effect size for all

Table 4. Meta-analysis of hot spots experiment effect sizes for main outcomes.

<i>Experiment</i>	<i>Effect size</i>	<i>Standard error</i>	<i>Inv. var. weight (% total weight)</i>	<i>95% C.I.</i>
Jersey City POP	2.05*	0.504	3.93 (1.8%)	Upper 3.04 Lower 1.06
Jersey City DMAP	0.689*	0.275	13.21 (6.0%)	Upper 1.23 Lower 0.15
Minneapolis Patrol	0.322*	0.142	27.15 (12.3%)	Upper 0.60 Lower 0.044
Kansas City Crack	0.219	0.139	51.32 (23.3%)	Upper 0.492 Lower -0.054
Minneapolis RECAP Commercial	0.089	0.127	62.49 (28.3%)	Upper 0.337 Lower -0.159
Minneapolis RECAP Residential	-0.009	0.127	62.49 (28.3%)	Upper 0.238 Lower -0.256
Meta-analysis All studies	0.345*	0.150	Total weight = 220.59	Upper 0.640 Lower 0.058
Meta-analysis w/o RECAP	0.632*	0.253	Total weight w/o RECAP = 95.61	Upper 1.13 Lower 0.138

* $P < 0.05$

studies. Overall, the hot spots policing interventions reduced citizen calls for service in the treatment places relative to the control places. The mean effect size for the hot spots policing intervention for the six studies was medium (0.345) and statistically significant.⁹ When the RECAP study was not included in the meta-analysis due to methodological concerns, the mean effect size was large (0.632) and statistically significant.¹⁰

The sensitivity of these findings to the selection of one effect size per study was examined by conducting a meta-analysis of all reported crime and disorder calls for service outcome measures across the studies. Table 5 presents the standardized mean difference effect sizes, standard errors, inverse variance weights, and 95% confidence intervals for 24 reported outcome measures. Of the 24 reported outcome measures in the hot spots experiments, 19 favored a treatment effect. Since the reported outcome measures in each study were not statistically independent effects, a mean effect size and inverse variance weight for each study was calculated based on all reported outcome measures within each study. The Q -statistic revealed that this distribution of effect sizes was homogeneous and, as such, a fixed effects meta-analytic model was used ($Q = 7.55$, $df = 5$, $P = 0.183$). As Table 5 reveals, the mean effect size for all reported calls for service outcome measures favored a treatment effect. However, in contrast to the meta-analysis of key reported outcomes, the mean effect size for all reported outcomes was smaller (0.129) and not quite statistically significant at the 0.05 level ($P = 0.0537$). When RECAP was not included in the meta-analysis, a fixed effects model ($Q = 3.08$, $df = 3$, $P = 0.379$) yielded a mean effect size for all reported outcomes that favored treatment (0.231) and was statistically significant ($P = 0.0225$).

Table 5. Meta-analysis of hot spots experiment effect sizes for all reported crime and disorder calls for service outcomes.

<i>Experiment and measures</i>	<i>Effect size</i>	<i>Standard error</i>	<i>Inv. var. weight</i>	<i>95% C.I.</i>
<i>Jersey city POP</i>				
Robbery calls	0.077	0.41	5.99	−0.723 to 0.878
Street fight calls	0.946*	0.43	5.39	0.102 to 1.79
Property calls	1.145*	0.44	5.16	0.282 to 2.01
Disorder/nuisance calls	0.1979	0.41	5.97	−0.60 to 1.00
Narcotics calls	1.302*	0.45	4.95	0.421 to 2.18
Total calls	2.05*	0.504	3.93	1.06 to 3.04
<i>Jersey city DMAP</i>				
Disorder calls	0.689*	0.275	13.21	0.15 to 1.23
Violence calls	−0.19	0.268	13.93	−0.71 to 0.335
Property calls	−0.059	0.267	13.99	−0.583 to 0.465
<i>Kansas City Crack</i>				
Disorder calls	0.164	0.139	51.43	−0.109 to 0.437
Violence calls	0.061	0.139	51.57	−0.211 to 0.334
Property calls	0.171	0.139	51.54	−0.102 to 0.444
Total calls	0.219	0.139	51.32	−0.054 to 0.492
<i>Minneapolis patrol</i>				
“Hard” crime calls	0.159	0.191	27.41	−0.215 to 0.5337
“Soft” crime calls	0.382*	0.192	27.0	0.005 to 0.759
Total calls	0.322*	0.142	27.15	0.044 to 0.60
<i>Minn. RECAP commercial</i>				
Disorder calls	0.086	0.127	62.44	−0.162 to 0.334
Property calls	0.162	0.127	62.30	−0.086 to 0.410
Personal crime calls	−0.132	0.127	62.36	−0.38 to 0.116
Total calls	0.089	0.127	62.49	−0.159 to 0.337
<i>Minn. RECAP residential</i>				
Disorder calls	0.024	0.126	62.49	−0.224 to 0.272
Property calls	−0.076	0.127	62.45	−0.323 to 0.172
Personal crime calls	0.174	0.127	62.26	−0.075 to 0.423
Total calls	−0.009	0.127	62.49	−0.256 to 0.238
<i>Meta-analysis – all outcomes</i>				
	0.129	0.067	Total weight = 222.42	−0.002 to 0.261
Disorder calls	0.161*	0.067	Total weight = 222.54	0.021 to 0.292
Property calls	0.098	0.071	Total weight = 195.44	−0.041 to 0.239
Property calls w/ hard crime	0.106	0.067	Total weight = 222.85	−0.025 to 0.237
Violence calls	0.043	0.070	Total weight = 195.81	−0.095 to 0.181
Violence calls w/ hard crime	0.057	0.066	Total weight = 223.22	−0.072 to 0.186

Table 5. Continued.

<i>Experiment and measures</i>	<i>Effect size</i>	<i>Standard error</i>	<i>Inv. var. weight</i>	<i>95% C.I.</i>
<i>Meta-analysis – all outcomes w/o RECAP</i>	0.231*	0.101	Total weight = 97.49	0.033 to 0.429
Disorder calls	0.297*	0.101	Total weight = 97.61	0.099 to 0.496
Property calls	0.197	0.119	Total weight = 70.69	–0.037 to 0.429
Property calls w/ hard crime	0.186	0.101	Total weight = 98.1	–0.012 to 0.384
Violence calls	0.048	0.119	Total weight = 71.19	–0.184 to 0.280
Violence calls w/ hard crime	0.079	0.101	Total weight = 98.6	–0.119 to 0.276

* $P < 0.05$.

The P -level for the “all outcomes” meta-analysis with RECAP included was $P = 0.0537$.

The reported outcomes were also aggregated into three categories of calls for service: disorder calls, property calls, and violence calls. Since the reported effects were not independent, a mean violence effect size was calculated for robbery and street fight call categories in the Jersey City POP experiment for inclusion in the overall violence calls meta-analysis. The Minneapolis Hot Spots Patrol experiment reported the effects of the intervention on “hard” crime and “soft” crime calls for service (Sherman and Weisburd 1995). The hard crime category included both property and violent crimes while the soft crime category included mostly disorder offenses. As such, the mean effect sizes for the aggregate property calls and violence calls categories were calculated with and without the hard crime call outcomes from the Minneapolis Hot Spots Patrol experiment. None of these effect size distributions were heterogeneous; therefore, fixed effects meta-analytic models were used to estimate mean effect sizes.¹¹

All estimated mean effect sizes favored a treatment effect for the hot spots policing programs across the specific call categories. With and without “hard crime” calls considered in the calculation of mean effect sizes, the treatment effects of hot spots policing on property and violence call categories were small and not statistically significant. However, hot spots policing interventions had a statistically significant treatment effect on disorder calls for service. When RECAP was included in the meta-analysis, the mean effect size was small (0.161). When RECAP was not included, the mean effect size was moderate (0.297).

Conclusion and policy implications

The results of this systematic review support the assertion that focusing police efforts at high activity crime places can be effective in preventing crime (Braga

2002; Eck 1997, 2002; Skogan and Frydl 2004; Weisburd and Eck 2004). Four of five experimental evaluations reported noteworthy crime and disorder reductions. Methodological problems in the research and evaluation design probably accounted for the lack of crime prevention gains in the Minneapolis RECAP experiment. Nonetheless, a meta-analysis of key reported outcome measures revealed a medium statistically significant mean effect size favoring the effects of hot spots policing in reducing citizen calls for service in treatment places relative to control places. While the estimated program effects were not as large, additional meta-analyses consistently reported mean effect sizes in favor of the hot spots policing treatment when all reported outcomes measures and specific crime categories of citizen calls for service were examined. Unfortunately, only two studies measured potential displacement and diffusion effects. When displacement was measured, it was very limited and unintended crime prevention benefits were associated with the hot spots policing programs.

It is important to note that these findings remain robust when the systematic review is expanded to include quasi-experimental designs. The earlier systematic review of experimental and quasi-experimental hot spots policing evaluations also found that focused police actions prevent crime and disorder in hot spots with little evidence of displacement (Braga 2001). The updated search for eligible studies in this review did not reveal any new quasi-experimental hot spots policing evaluations. Interestingly, the most powerful effects in favor of the hot spots policing treatment were associated with experimental evaluations. In the previous review, quasi-experimental evaluations that favored treatment were more likely to report weaker program effects.¹² This finding contrasts with a recent examination of the relationship between research design and study outcomes in a review of research evidence on crime and justice commissioned by the National Institute of Justice that found quasi-experimental designs, relative to randomized trials, were more likely to report results in favor of treatment (Weisburd et al. 2001).

Unfortunately, the results of this review provide criminal justice policy makers and practitioners with little insight on what types of policing strategies are most preferable in controlling crime hot spots. Clearly, the enforcement-oriented strategies reviewed here work in preventing crime. We do not know, however, which enforcement strategies are more effective in preventing crime and under what circumstances are certain strategies more appropriate. This review also offers little insight on the effectiveness of enforcement tactics relative to other broader-based community problem-solving policing programs (see, e.g., Skogan and Hartnett 1997). This small body of evaluation research does not unravel the important question of whether enforcement-oriented programs result in long-term crime reductions in hot spot areas. Research suggests that a variety of situational factors cause crime to cluster at particular places (Eck and Weisburd 1995). Proactive patrols, raids, and crackdowns do not specifically address the site features and facilities that cause specific locations to generate high volumes of crime. With the exception of the problem-oriented policing programs with limited situational interventions, the place-oriented interventions in this review consisted of uniform tactics applied across heterogeneous places. Perhaps a greater focus on changing

these criminogenic situational characteristics would result in longer lasting crime reductions at crime places.

Beyond thinking about the relative crime prevention value of these programs, we need to know more about community reaction to increased levels of police enforcement action. Police effectiveness studies have traditionally paid little attention to the effects of policing practices upon citizen perceptions of police legitimacy (Tyler 2000, 2001). Does the concentration of police enforcement efforts lead citizens to question the fairness of police practices? There is some evidence that residents of areas that are subjected to hot spots policing welcome the concentration of police efforts in problem places (Shaw 1995). Nonetheless, focused aggressive police enforcement strategies have been criticized as resulting in increased citizen complaints about police misconduct and abuse of force in New York City (Greene 1999). The potential impacts of hot spots policing on legitimacy may depend in good part on the types of strategies used and the context of the hot spots affected. Whatever the impact, we need to know more about the effects of hot spots policing approaches on the communities that the police serve.

Acknowledgements

This research was supported through funds from the Smith Richardson Foundation and the U.S. National Academy of Sciences. David B. Wilson deserves special thanks for his analytic support (and patience) in the completing the meta-analysis. The author would also like to thank Phyllis Schultze of Rutgers University's Criminal Justice Library and Deborah Braga of the University of Massachusetts-Amherst for their assistance in searching for and locating eligible studies. David Weisburd, Larry Sherman, Mark Lipsey, Anthony Petrosino, Brandon Welsh, and David Farrington also deserve thanks for making helpful comments on earlier iterations of this review.

Notes

- 1 Braga (2001) identified four quasi-experiments evaluating the effects of hot spots policing on crime. Only one study, the Kansas City (MO) Gun Study (Sherman and Rogan 1995b), reported the necessary information to properly calculate a program effect size. Two studies, the St. Louis Problem-Oriented Policing in Three Drug Market Locations Study (Hope 1994) and the Beenleigh (Australia) Calls for Service Project (Criminal Justice Commission 1998), did not report the necessary information to calculate program effect size. The Houston (TX) Targeted Beat Program (Caeti 1999) did not use appropriate statistical methods to estimate program effects and, unfortunately, an accurate effect size could not be calculated (see Braga 2001 for a discussion).
- 2 In 2001, the earlier systematic review identified 588 distinct abstracts and the full-text reports, journal articles, and books for 43 abstracts were acquired (Braga 2001). The search results of the earlier review were included in the 2003 review.
- 3 One case was excluded from these analyses because the observational data were inappropriately collected (Braga et al. 1999).

- 4 One case was excluded from these analyses because it did not have any physical disorder in the pre-test and post-test periods (Braga et al. 1999).
- 5 The landmark Kansas City Preventive Patrol Experiment had to be stopped and restarted three times before it was implemented properly; the patrol officers did not respect the boundaries of the treatment and control areas (Kelling et al. 1974). Likewise, the design of the Minneapolis Spouse Abuse Experiment was modified to a quasi-experiment when randomization could not be achieved because officers chose to arrest certain offenders on a non-random basis (Berk et al. 1988).
- 6 Property crime incidents experienced a significant increase while property crime calls for service did not significantly change in the treatments catchment areas relative to controls. The research team viewed this result as an artifact of the experiment rather than a substantive finding (Braga et al. 1999).
- 7 The SPSS meta-analysis macro developed by David B. Wilson was used to conduct these analyses (<http://www.mason.gmu.edu/~dwilsonb/home.html>).
- 8 The random effects variance component reported here was calculated using the method of moments technique and the overall results do not differ substantively when the full information maximum likelihood technique is used (Mean ES = 0.322, SE = 0.135, $P = 0.0168$, variance component = 0.06737). Using a fixed effects model, the mean effect size for the key outcome measures is smaller, but remains statistically significant (Mean ES = 0.191, SE = 0.067, $P = 0.0045$).
- 9 The modest difference between the mean effect sizes reported by the fixed effects and random effects models suggests a possible relationship between study size and effect size with the smaller studies having the large effects. This is often seen as evidence of "publication bias" (i.e., unpublished studies not included in review) that requires a "trim-and-fill" analysis to estimate how many studies have not been observed and adjust for missing effect sizes (see, e.g., Duval and Tweedie 2000). A trim-and-fill analysis is overly conservative in this meta-analysis for two reasons. First, this review includes unpublished studies. Second, there is strong reason to believe that the relationship between sample size and effect size also reflect implementation problems. As described earlier, the RECAP experiment, which accounts for two studies (commercial places and residential places) with large weights in this analysis, suffered from serious implementation problems that influenced the evaluation findings. When a trim-and-fill analysis is run on these data, the random effects model does not generate substantive changes to the findings reported here (results are available upon request from author).
- 10 The Q statistic was statistically significant ($Q = 13.6365$, $df = 3$, $P = 0.0034$). Therefore, a random effects meta-analytic model was used ($v = 0.182234$).
- 11 When RECAP was included, the Q statistic was not statistically significant for disorder calls ($Q = 6.5341$, $df = 5$, $P = 0.2577$), violence calls ($Q = 4.97$, $df = 4$, $P = 0.2901$), and property calls ($Q = 8.42$, $df = 4$, $P = 0.0773$). When "hard" crime calls for service were included in the meta-analysis, the results did not change for violence calls ($Q = 5.37$, $df = 5$, $P = 0.3725$) and property calls ($Q = 8.51$, $df = 5$, $P = 0.1303$). When RECAP was not included, the Q statistic was not statistically significant for disorder calls ($Q = 3.1933$, $df = 3$, $P = 0.3628$), violence calls ($Q = 2.023$, $df = 2$, $P = 0.3637$), and property calls ($Q = 5.589$, $df = 2$, $P = 0.0611$). When "hard" crime calls for service were included in the meta-analysis without RECAP, the results did not change for violence calls ($Q = 2.267$, $df = 3$, $P = 0.5189$) and property calls ($Q = 5.617$, $df = 3$, $P = 0.1318$).
- 12 The three quasi-experiments that favored a treatment effect were the Kansas City (MO) Gun Project (Sherman and Rogan 1995b), St. Louis Problem-Oriented Policing in Three Drug Market Locations Study (Hope 1994), and the Houston (TX) Targeted Beat

Program (Caeti 1999). The Beenleigh (Australia) Calls for Service Project did not report any significant differences in the total number of calls for service between the treatment and control jurisdictions (Criminal Justice Commission 1998). The Kansas City Gun Project, which had the strongest quasi-experimental design, reported a statistically significant 49% decrease in gun crimes in the target beat relative to the control beat. The St. Louis study reported varying reductions in total calls for service on blocks at the three drug market locations that were larger than reductions observed in blocks located in surrounding areas. However, there was significant displacement from one of the treatments locations to the surrounding addresses. The Houston study reported significant reductions in index crimes in aggregated treatment beats relative to aggregated control beats, but mixed findings in particular beats.

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